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ABSTRACT

The University Council for Educational Administration (UCEA) ought to reaffirm its commitment to theoretical research. Research in educational administration during the last twenty years has been applied or practical rather than purely theoretical or scientific. Although there is certainly a place for practical research, theoretical research ought not be neglected. One goal of theoretical research is to develop theory that can explain phenomena. Another goal is to add to the store of knowledge. Research not based on a body of substantive theory will be weak. Purely practical research is often fragmented and unsystematic. In the late fifties UCEA inspired a movement toward more interdisciplinary and scientific analysis of educational organizations. This so-called "theory" movement declined in the late sixties partially because of institutionalization and overpromotion. The social and political unrest of the sixties and the financial exigencies of the seventies caused practical and policy research to dominate the field. Yet more scientific and theoretical research is necessary for a full knowledge and understanding of organizational behavior in education. UCEA must continue to be an advocate of such research. (JM)

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Scientific THEORY RESEARCH PRACTICE in Educational Administration

by
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EA 010 783

Editor's Note: This paper is a slightly adapted version of Wayne K. Hoy's Presidential Address, delivered at the Annual Meeting of UCEA in Atlanta, Georgia in February, 1978.

UCEA should strongly support theory-based research. It has been suggested that UCEA emphasize practice and applied research at the expense of scientific research that expands theoretical knowledge. I believe that course is short-sighted and unsound; I believe UCEA can serve the discipline and preserve its own uniqueness by reaffirming its commitment to theoretical or scientific research.

Scientific Research

Today, I am going to talk about scientific research, using the term as Kerlinger does: scientific research refers to the systematic and critical empirical investigation of hypothetical propositions.[1] In this definition, the purpose of research is to develop theory, and, of course, the purpose of theory is to understand and explain phenomena. Admittedly, this is a rather narrow conception of the term. So be it.

I am not especially interested in historical research, action research, policy research, decision-

oriented research, or applied research. Nor am I particularly concerned with the practical idea that research should pay off and provide a guide to social action and policy. Furthermore, I reject the notion that scientific research excludes such field study methodologies as participant observation, case study, or comparative studies.[2] Lipset's[3] classic case study of the International Typographical Union and Blau and Scott's[4]comparative analysis of social welfare agencies provide two good examples of field studies that were hypothesis-testing investigations aimed at theory development. The point is that scientific research is not wedded to any one research design or methodology; it may be a sample survey, a controlled experiment, or a field study.

The field of educational administration is oriented toward practice. It always has been and probably always will be. After all, as professors of educational administration, we are concerned first and foremost with preparing practitioners. It is not surprising that practical problems, social issues and the need for relevancy dominate the writing, teaching, and research in educational administration. However, this pragmatic-practical viewpoint espoused by professors limits the production of scientific research. Most professors assume that research can solve educational problems and improve administrative practices. The assumption is false. Most research does not lead *directly* to improvement in administrative practice *nor should we hold such expectations.*[5]

Take a rather straight forward example: The pupil control orientation of schools has been directly linked to increased levels of alienation among high school students; the more custodial the climate of the school, the more the students suffer from a sense of powerlessness.[6] The implications, at first blush, seem apparent: reduce the custodialism in pupil control orientation of teachers.

Unfortunately, things are not that simple. How can one change the control orientation of teachers? Should one hire only humanistic teachers? Research also shows that beginning, humanistic teachers quickly become more custodial as they become socialized by their experienced colleagues.[7]. Problematic as changing the control orientation of a school is, if we could do it, we would confront still another host of problems. How is custodial pupil control related to other important aspects of student sense of alienation? Although custodialism in schools is directly related to powerlessness, for example, it is inversely related to meaninglessness.[8] It also seems likely that some students will perform better academically with custodial teachers and others with humanistic teachers. Implicit in our discussion has been the assumption of unidirectional causality between pupil control orientation and student characteristics, a somewhat tenuous assumption at best.

So what appeared to be a research finding with some clear cut implications for practice, turns out to be quite removed from practice; indeed it raises more questions than it answers. Such is the character of scientific research. Kerlinger succinctly describes the dilemma:

> Studying relations and taking action are on two different levels of discourse which one cannot easily bridge. Scientific research never has the purpose of solving human or social problems, making decisions, and taking action.[9]

The practice of science is a way of knowing things and requires a faith that it is a valid approach to knowledge and an assumption that the nature of reality is ultimately material and knowable. Practiced rigorously or honestly, scientific research can be embedded in any culture or any time and produce reliable findings. Sometimes the findings may be wrong. The theory may be in error, the instruments poor, the design inadequate. Still, when the findings are added to the pool of information, further testing, further inquiry will refine the gold from the dross.

We should not lose faith in our ability eventually to understand a great deal about organizations. Our attempts to describe have been a slow-going affair, indeed, but is that not to be expected? Mathematics labored centuries to discover the zero. Given the immense number of variables in human organization, one is surprised that we have gone as far as we have, and we have only just begun. Many studies being done now, particularly those using sophisticated statistical treatment, simply were not possible until quite recently.

If we give up this tool, this way of knowing things, this basic, empirical investigation, what will we have in its place? Once again the timeless will have been bargained away for the timely. Once again the products of honest inquiry will be rooted into the transient ethos of a particular culture where they will erode over the passage of time either to be preserved by historians or "buried in the black night of the utterly forgotten."

Perhaps I overstate the case. There is, to be sure, a need for other kinds of knowledge. I am *not* arguing that the only research worth doing is hypothesis-testing study to confirm theory. There is a growing need for sound policy research aimed at guiding social action, and good applied research certainly is useful. But given the practice-orientation of the field of educational administration, there is little need to speak out on the importance of practice, relevance, or utility. We are bombarded from all directions with exhortations to make our teaching and research more useful and pragmatic. We are criticized by practitioners, state and federal officials, congressional leaders, and even by our own colleagues for what they consider a neglect of pressing problems and immediate social issues.

The press for the practical continues to grow. Programs of support, both in foundations and government, are now concerned primarily with grants to improve practice and return immediate payoffs. As a congressional staff member remarked last year,

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"We want N.I.E. to show us that we are getting a bang for the bucks we are spending on educational research."[10] A former governor of one of our most populous states opposed the funding of basic research at the state university because it would just be "subsidizing idle curiosity." In 1975, Jack Getzels cogently warned of some of the inherent dangers of this perspective:

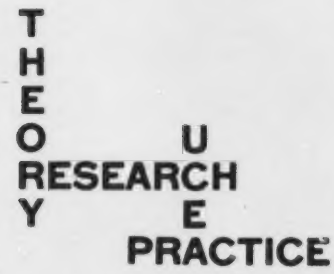
> Today, there is pressure on the university and on certain elements in it, of which educational administration is a prime target, to turn away from exploring fundamental enigmas and to deliver technical services, to advocate policies rather than formulating problems, and to deal with practical necessity rather than with conceptual uncertainty. The pressure has its source not only in the real difficulties facing education, but in an unhappy Confluence of Anti-intellectualism and financial stringency; it takes many forms—the call for assembly-line efficiency, the applications of business-like management, and above all, the accounting of results not by their contribution to fundamental and ultimate knowledge but by their immediate and material consequences.[11]

Scientific research is basically problem-generating rather than problem-solving. The work of the researcher lies distinctively in exploring problems that are ultimate rather than immediate, and fundamental rather than pragmatic. The researcher, then, is particularly vulnerable in a time when accountability and payoff are stressed. To the scientific researcher the disinterested pursuit of understanding and explanation for its own sake is reason enough to engage in systematic study. Yet the cry remains, "Knowledge for what?" Given the criteria of practice, relevancy, and utility that pervade the field of educational administration, it is no wonder that study done in educational administration is overwhelmingly *not* scientific research.

Research in Educational Administration

A number of recent studies have examined the status of research in some detail.[12] They report that research in educational administration has become slightly more rigorous and scientific, but there is little question that during the past two decades it has maintained a practical emphasis—that is, there continues to be a preoccupation with practical relevance in the field.

Research and scholarly writing are *not* primary interests of most professors. Not only do professors



spend very little time doing research, they typically have no strong desire to do much more research. Campbell and Newell estimate that the average investment in research and scholarly writing is only about 10 percent. I might add that, had the definition of research been limited to empirical investigations of hypothetical propositions, it seems safe to predict an even more drastic reduction of research interest and productivity among professors of educational administration.

Immegart [14] concludes from his study, probably the most recent comprehensive evaluation, that most of the problems identified by Griffiths[15] in his 1959 and 1965 analyses of research remain with us today. Most research in the field is still done by students, and usually it is poorly done. The practice orientation of most researchers, professors as well as students, leads to a preoccupation with practical problems and immediate results, a perspective that I have argued undermines scientific research.

Closely related to an inadequate research orientation is the weakness of research that is not anchored in theory. Theory is both the beginning and the end of scientific research. On the one hand, theory serves as the basis for generating hypotheses to test verifiable propositions that describe and predict observable empirical phenomena. On the other hand, the ultimate objective of all scientific endeavor is to develop a body of substantive theory.[16] Without sound theoretical bases for our research, we shall continue to flounder and to carry out what masquerades as research. Descriptions of practice devoid of relations among variables is not scientific research. So called policy research in which the task is to find data to support an existing policy is not simply poor research, it is shoddy practice.

Finally, a review of the *Educational Administration Quarterly* or the *Journal of Educational Administration* reveals several other indictments of research in the field. There are virtually no significant programmatic efforts in the study of educational administration. The research is fragmented and lacks a systematic attack on a series of related problems. There is little in the way of replication, improving, or building on others' work. We rarely base new work on existing work. Critical analyses and scholarly exchanges on research are conspicuously absent from the literature.

Despite dire predictions of the demise of theory and research in the field,[18] there has been gradual

progress in the past two decades. Though the work is varied and uneven, the weight of available evidence suggests that research in educational administration not only has increased in quantity but also has shown some improvement in quality.[19] However, the advancement has been halting and modest at best.

Research, Theory, and UCEA

UCEA has had its impact on research in the field. It co-sponsored the seminar on Administrative Theory in Education in 1957, an auspicious event for those interested in raising the standards of research. That seminar served as a catalyst for a new movement that rejected naked empiricism and favored an interdisciplinary and scientific analysis of educational organizations. A testable knowledge base would replace anecdote and prescription. A number of leading professors in UCEA universities turned their attention to scientific research. Many young, research-oriented professors were recruited by UCEA universities in the late fifties and early sixties. They were attracted by an intellectual and academic ferment fostered by UCEA. There is little question that UCEA Played an important role in the movement and in the subsequent stimulation of research and theory in the field.

The establishment by UCEA of the *Educational Administration Quarterly* in 1965 marked the introduction of the first scholarly research journal in the field. It provided research-oriented professors with a forum to present, discuss, and critique theory and research. Shortly following the *Quarterly,* UCEA introduced the *Educational Administration Abstracts.* a vehicle for codifying research germane to the field. Both journals significantly aid scholarly exchange, and both remain important, lasting contributions, despite the fact that the interdisciplinary theory research movement ran into strong opposition in the late sixties.[20]

The decline of empirical, theory-based research aimed at knowledge production can be linked to the decline in the so-called theory movement.[21] As with most new movements, its life history had three major phases: enthusiasm, vulgarization, and institutionalization.[22] The enthusiastic, but deliberate, support of the original innovators quickly gave way to the promoters who jumped on the band wagon to exalt the movement, conjuring up unrealistic illusions of outcomes. Finally, the movement became institutionalized, a time of ritualization in which the imitation of innovation, the charade of the scientific, was incorporated into the standard program. Can you think of one respectable program in educational administration that doesn't have its required course in theory and research? At any rate, as institutionalization takes root, the once new idea then became a candidate for displacement by the next "big deal" to come along. Pointedly, Iannaccone observed that "each year of research in educational administration is better predicted by a content analysis of the *Saturday Review* or some other widely distributed periodical than any set of academic journals."[23]

The era of grantsmanship in the late sixties saw support turned away from basic research to development, dissemination, and applied research, research that would have immediate practical pay-off. Scientific research never did get a firm footing in the field. The research professorship in educational administration remains basically a non-role with less than two percent of professors spending more than fifty percent of their time on research.[24]

The movement was overpromoted, and extravagant expectations quickly gave way to disdainful disillusionment. There were benefits from the attention given to interdisciplinary theory and research. Textbooks, instructional materials, new courses, and new professors all bear the mark of the effort. However, a field dominated by practice-oriented professors skeptical, if not resistant, to theory and research, when confronted by the social and political unrest of the sixties and the financial and political exigencies of the seventies, quickly retreated from theory-directed research. Civil rights and inequality, Selma and Vietnam, activism and confrontation, accountability and management of decline, polarization of educational issues—all impinged on the study and practice of educational administration. The tenor of the times is practice, action, and immediate results, not theory, research, and reflection.

There are more researchers in the field now than ever before, but still not many. The young, research-oriented professors drawn to the field in the early sixties are dispersed rather than concentrated, though public forums and vehicles for dialogue and exchange exist. Scientific research and theoretical analysis have given way to a dominant motif of "practical" research and utility. Yet we have made some advancement in theory and research during the past two decades and that progress continues although slowly. The picture of research in educational administration, while bleak, is not hopeless.

Scientific Research and Practical Research

Perhaps a more fundamental issue is the question of balance between basic scientific research that seeks new knowledge and practical research that has immediate utility. In evaluating the current "state of the field," Culbertson concluded, "The sixties likely overemphasized research on more basic questions

and neglected the more visible and immediate problems. There is a distinct danger that in the 1970-77 period the emphasis has gone too far in the other direction."[25]

The danger of the seventies is upon us, a resurging cult of immediacy. I seriously question whether or not the sixties produced an overemphasis on scientific research at the expense of practical considerations. To the contrary, I suspect that the pervasive and entrenched practice orientation of the professoriate prevents any substantial movement toward scientific research.

According to Immegart, scientific research in the field peaked out in the mid-sixties.[26] Certainly, the peak was a modest one. We need not worry about the field becoming too theoretical and research-oriented; the norms for practice and utility are too strong. The *Educational Administration Quarterly* remains the only quarterly I know of that is published three times per year. This in large part reflects the lack of quality research concerned with the expansion of knowledge basic to an improved understanding of administrative behavior in complex organizations.

The seventies bear witness to the vitality of the practice orientation. There is a visible press of focus on practice. A press to train leaders to *practice*, to to perform research to inform *practice* and to make decisions to shape *practice*, a press for development and for practical research.[27] That press, if it continues unabated, may well nullify the modest gains we have made in administration. Recently, the demand for professors with general training in theory and research has diminished. In many universities, the number of students in residence has declined, and there is a trend toward field-based preparatory programs.[28] None of these events bodes well for basic study in educational administration.

The press for practical research also manifests itself in the rising popularity of policy research, that is, research that provides a guide to social action.[29] The goal is not to develop theory but to gain information for social action and policy decisions. James Coleman forcefully argues that the university setting is unsuited for conducting policy research.

First, the university is dedicated to an open publication of research results. Research problems are, as it were, owned by the discipline and subject to public scrutiny and increasing refinement through the process of investigation by many researchers. Policy studies are quite different in that they often require a confidentiality between client and researcher, and recommendations are not subject to a test throughout the discipline. Perhaps, this is just as well for all involved.

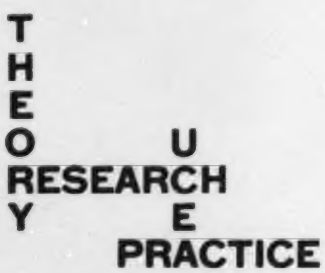
Secondly, the flat organizational structure of the university does not foster policy research. The bureaucratic authority of the university tends to regulate teaching rather than policy studies. As a consequence the research is not held to a timeline for the production of findings. Moreover, the investigator may modify and distort the policy problem, so that research does not address the original problem, but addresses instead a problem of more direct interest to the discipline or to the investigator, or one easier for him [her] to carry out."[30] In brief, it appears that for at least two reasons the university, in general or in specific departments, is not well suited for policy research. Nonetheless, the push for research in that direction continues.

The emphasis on the practical discriminates against basic inquiry into the nature and dynamics of organizations when that research has no immediate application. In such a situation, the researcher must justify his/her investigation not according to the canons of methodology, but according to a standard of applied utility. Clearly, no direct justification is possible since the nature and aim of scientific research is different from applied research. In the face of a persistent demand for immediate and practical consequences of research, the quantity and quality of scientific research will decline. At the risk of being redundant, I must say again that the aim of scientific research is not utility. It is knowledge. If this is not understood, what applications are made will be based on anecdote, rather than tested theory. In the long run I don't believe anyone wants that.

Conclusion

Scientific research in educational administration needs a voice; it needs an advocate. UCEA could fulfill that need. Currently, scientific research is not popular. Yet I believe it is essential if our progress in educational administration is to continue, even at a slow and deliberate pace. UCEA was an instrumental force in facilitating and stimulating interest and action in theory and research in the fifties and sixties. The seventies have seen UCEA turn first toward "knowledge utilization" and more recently toward "a dual mediating function—between theory and practice and between professors and practitioners" as themes for planning and action. Clearly these directions are practice-oriented, yet UCEA remains concerned about theory and research.

At a career development seminar on research in educational administration at the University of →



Rochester this past spring, a UCEA Research Commission was developed to devise strategies to improve the quality of research production within the field. Plans are underway for the development of seminars on the evolution of the sociology of science and on research methodology. The commission is hopeful of convincing the editorial board of the *Educational Administration Quarterly* to publish a review of research in educational administration on a regular basis. Nevertheless, I am a little uneasy about the future of research.

I am not arguing against applied or practical research. Such research undoubtedly has its positive effects on practice, but I believe that its potential for long-lasting and far-reaching effects is not as great as that of basic scientific research. We need all the quality research we can get. The issue is not either applied, practical research or basic, scientific research. We need both! What I am seriously concerned about is that our re-emphasis of practice, relevance and utility in educational administration will lead to a diminution of scientific research. It seems quite plausible that under the present pressure for immediate results, and *unless deliberate precautions are taken to guard against it, applied and practical research will invariably drive out more basic, scientific research.* Such a consequence would be a major frustration to knowledge development.

I submit that UCEA has the capability to help guard against the demise of scientific research in educational administration, but it needs to remain ever alert to the danger of becoming seduced by the sirens of practice. UCEA must continue to be a convincing advocate of scientific research in educational administration. It is not in vogue to pursue research simply for its own sake; however, such a pursuit is indispensable to a fuller knowledge and understanding of organizational behavior in education. □

NOTES

1. F.N. Kerlinger, *Foundations of Behavioral Research* (New York: Holt Rinehart and Winston, 1973), p. 11.
2. L. Iannaccone, "Interdisciplinary Theory Guided Research in Educational Administration: A Smoggy View From the Valley," *Teachers College Record* 75 (September, 1973), pp. 55-66.
3. S. Lipset et al., *Union Democracy* (Glencoe, Ill.: Free Press, 1956), p. 413. Actually the study was not only an hypothesis-testing study seeking to test specific propositions about the occupational community of printers and its relation to union politics, but it becomes hypothesis-generating when an attempt is made to formulate propositions "aimed at identifying the factors which make for and sustain democracy in private organizations."
4. P. M. Blau and W. R. Scott, *Formal Organizations* (San Francisto: Chandler Publishing Company, 1962).
5. Kerlinger takes the same position for all educational research. See F. N. Kerlinger, "The Influence of Research on Education Practice," *Educational Researcher* 6 (September, 1977), pp. 5-12.
6. W. K. Hoy, "Dimensions of Student Alienation and Characteristics of Public High Schools," *Interchange* 3 (1972), pp. 38-52.
7. W. K. Hoy, "The Influence of Experience on the Beginning Teacher," *The School Review* 76 (September, 1968), pp. 312-323; W. K. Hoy, "Pupil Control Ideology and Organizational Socialization: A Further Examination of Experience on the Beginning Teacher," *The School Review* 77 (September, 1969), pp. 257-265.
8. Hoy, "Dimensions of Student Alienation and Characteristics of Public Schools," pp. 38-52.
9. Kerlinger, "The Influence of Research on Education Practice," p. 6.
10. I. E. McNett, "R & D Can Help With the ABC's" (July 18, 1976), *New York Times*.
11. J. W. Getzels, "Educational Administration Twenty Years Later, 1954-1974," in L. L. Cunningham, et al. (eds.), *Educational Administration: The Developing Decades*, (Berkeley, California: McCutchan Publishing, 1977), p. 21.
12. G. L. Immegart, "The Study of Educational Administration, 1954-1974," in L. L. Cunningham, et al. (eds.) *Educational Administration: The Developing Decades*, pp. 298-328; and R. F. Campbell and L. J. Newell, *A Study of Professors of Educational Administration*, (Columbus, Ohio: UCEA, 1973).
13. Ibid.
14. Immegart, "The Study of Educational Administration, 1954-1974."
15. D. E. Griffiths, *Research in Educational Administration: An Appraisal and a Plan* (New York: Bureau of Publication, Teachers College, Columbia University, 1959); D. E. Griffiths, "Research and Theory in Educational Administration," in W. W. Charters, Jr., et al. (eds.), *Perspectives on Educational Administration and the Behavioral Sciences* (Eugene, Oregon: Center for the Advanced Study on Educational Administration, 1965) pp. 25-48.
16. For a more detailed analysis of the relation of theory, research, and practice, see W. K. Hoy and C. G. Miskel, *Educational Administration: Theory, Research, and Practice* (New York: Random House, 1978). For an excellent general analysis on theory in educational administration, see D. J. Willower, "Theory in Educational Administration," *Journal of Educational Research* 13 (May, 1975), pp. 77-91.
17. For a discussion of legitimate policy research, see James S. Coleman, *Policy Research and the Social Sciences* (Morristown, N.J.: General Learnings Press, 1972).
18. A. W. Halpin, "Administrative Theory: The Fumbled Torch," in A. M. Kroll (ed.), *Issues in American Education* (New York: Oxford University Press, 1970), pp. 156-183; A. W. Halpin and A. E. Hayes, "The Broken Ikon, or, What Ever Happened to Theory?" in L. L. Cunningham, et al. (eds), *Educational Administration: The Developing Decades*, pp. 261-297.
19. Immegart, "Study of Educational Administration, 1954-1974."
20. For more extensive analyses of the so-called theory movement in educational administration, see Halpin, "The Fumbled Torch"; Halpin and Hayes, "The Broken Ikon"; Iannaccone, "Interdisciplinary Theory Guided Research."
21. Immegart, "The Study of Educational Administration, 1954-1974," p. 316.
22. H. A. Thelen, "New Practices on the Firing Line," *Administrators Notebook* 7 (January, 1964), pp. 1-4.
23. Iannaccone, "Interdisciplinary Theory Guided Research in Educational Administration: A Smoggy View From the Valley," p. 58.
24. Campbell and Newell, *A Study of Professors of Educational Administration*, p. 57.
25. J. A. Culbertson, "An Exploratory Discussion of Mission Related Objectives," mimeographed, Columbus, Ohio: UCEA, September, 1977.
26. Immegart, "The Study of Educational Administration, 1954-1974," p. 316.
27. Culbertson, "An Exploratory Discussion of Mission Related Objectives."
28. Ibid.
29. Coleman, *Policy Research and the Social Sciences*.
30. Ibid, p. 20.